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Comment on cp-2021-122

Peter Bijl (Referee)

Referee comment on "Late Paleocene CO₂ drawdown, climatic cooling and terrestrial denudation in the southwest Pacific" by Christopher J. Hollis et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-122-RC1>, 2021

General comments

The authors present a really compelling dataset representing local depositional setting and terrestrial climate of the Paleocene of New Zealand. The geographical spread of the records, around the north and eastcoast of the continent, makes it a comprehensive and complete overview, with compelling implications. I have some comments on the way the study is introduced and discussed, but these should be easily fixable, either as reply or in a revised draft.

I understand the introduction gives the potential importance of understanding Waipawa organofacies deposition in the context of past climate change, with CO₂ drawdown as mechanism and that would fit well with the scope of the journal. However, given the primary focus of the study, to characterize the black shale OM content, and understand the enigmatic enrichment in ¹³C, I would suggest the authors focus the introduction a bit more on existing investigations in other black shales. As it is now, the reader expects a "CO₂ drawdown paper" but gets quite detailed analyses of OM composition and geochemistry instead. Meanwhile, the quantification of CO₂ drawdown and a convincing argumentation for why the found signals can only be caused by CO₂ drawdown, is largely missing. Assessing the way the aims of the paper are introduced may be a bit outside my tasks as a reviewer, but I feel the way it is now has the introduction somewhat disconnected to the bulk of the paper.

The aim of the study is to find the cause of the ¹³C-enriched OM. The authors argue for CO₂ drawdown as a cause, and indeed that could be one of the reasons (although there are some others as well). However, the authors add cooling as supporting argument for that (it is cooling, so there must have been a CO₂ decline), and I think this drives down a dangerous road towards circular reasoning. First of all, they drive away from all the possible other reasons other than CO₂ drawdown of why this region cools. Evidence of Paleocene cold conditions mostly comes from southwest Pacific SST data, which represent at best local signals. The authors mention another reason for regional cooling themselves: increased upwelling of deep water. Benthic foram records might be biased by an unknown

amount of ephemeral ice volume, and cannot be taken as paleotemperature proxy as such. Secondly, if the cooling is indeed global, the relation to radiative forcing has the issue that long-term trends in benthic foram $\delta^{13}\text{C}$ (representing carbon cycle) and $\delta^{18}\text{O}$ (representing temperature/ice volume) are out of phase by 1.5 Myrs. Westerhold et al., 2011 provides dissolution as a potential but uncertain reason for this, but as long as this is unresolved, the community has to entertain the idea that this represents a genuine signal, with understanding of the 1.5myr delay unexplained. Then, If the abstract and the rest of the paper reads as if it was shown that CO_2 drawdown caused the $\delta^{13}\text{C}$ enrichment, people will use the paper as evidence for CO_2 decline in the Paleocene, while actually that conclusion was drawn with the use of (local) SST decline as argument. Then CO_2 reconstructions and temperature reconstructions have lost their independence, which is a tricky road.

Other factors may explain why $\delta^{13}\text{C}$ of higher plants might be shifting carbon isotope values over these time scales: lapse rates, for instance (Körner et al., 1988; doi: 10.1007/BF00380063). Could the authors find evidence to exclude the possibility that a change in altitude of the catchment caused some of the $\delta^{13}\text{C}$ excursion in the terrestrial components? I feel that the authors should more carefully exclude other arguments to explain the changes in $\delta^{13}\text{C}$ before the conclusion is drawn that CO_2 drawdown caused it. This means acknowledging other potential factors.

Another (in my mind) obvious omission in the paper is the implications of the reconstructed intense river runoff signal in the records for local paleogeography and paleoenvironments. Many records of the Waipawa organofacies come from the east coast of NZ, which today, owing to a high mountain range and prevailing westerly winds, is in an intense rain shadow. The observation of intense river runoff in the Paleocene on the eastcoast of NZ could mean 2 things: (1) prevailing easterly winds in the Paleocene, which is unlikely, but could be verified in model simulations (2) absence of a rain shadow, which means absence of a strong mountain divide. I believe this must be discussed in the paper, and because the evidence for intense runoff is way clearer than the link to atmospheric CO_2 drawdown, I would suggest the authors focus their paper towards the implications for local paleogeography, hydrology and paleoenvironment.

Comments in chronological order

Abstract line 25–27: Authors should specific about trends vs peak values (cooling versus cold). The 1.5 million year offset means that it is crucial that the authors place the timing of deposition of the Waipawa organofacies and the SST trends relative to the carbon isotope maximum and the oxygen isotope maximum. To me, “cooling” refers to a decreasing trend in temperature, rather than a temperature minimum. Does the deposition of the Waipawa organofacies now coincide best with the benthic foram $\delta^{13}\text{C}$ trends, the $\delta^{18}\text{O}$ maximum or with the SST minimum? Some careful rewording might be needed here to make it really clear.

Line 109: the SST data of ODP Site 1172 are indeed published by Hollis et al., 2014, but note that these were updated in Bijl et al., 2021 with higher resolution, and beyond TEX86. Moreover, the primary source for the organic $\delta^{13}\text{C}$ data is Röhl et al., (2004; Geophysical monograph series 151). This should be acknowledged.

What I would suggest, is that the authors add a small plate presenting wide brightfield microscope images of the palynofacies, highlighting the main palyno groups. It might sound obvious for people working with palynofacies, but given the importance of this dataset for the story, I feel some visual support is warranted.

Line 435–439: See Komar et al., 2013 <https://doi.org/10.1002/palo.20060>, attempting to reconcile the long-term trends in carbon and oxygen isotopes, and lysocline evolution using a carbon cycle box model.

The inshore-to-offshore trends in Fig. 14 are all but compelling. There is also a good reason why: global average sea level was really low, which means accommodation space was reduced. If the transect does not include sites off the slope (and Mead Stream is top slope, if I am correct), you will not find much of a transect when terrestrial input is so intense.

In section 6.4, I am losing the connection to the new results. I propose the authors revisit this section to see how it can be more closely connected to their results and implications.